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The Editor types these notes at the end of what has perhaps been the busiest week of a very busy term (to be correct he should say, at the beginning of the week following, since he does so on Sunday - 'the better the day the better the deed' - indeed on 'Stir-up Sunday', the first in Advent and that on which Christmas puddings are traditionally made, in accordance with the injunction in the collect for the day)\*; and late at night too so that he should perhaps apologize in advance for any errors that creep in, and had better start with the traditional greeting - A HAPPY CHRISTMAS TO ALL OUR READERS, though this number is not a particularly Christmassy one, nor does it contain as many articles as has recently been the practice. What it does contain is something that he had thought might become a regular feature, a *Forschungsbericht* for the edification rather than the entertainment of readers, and on a subject which is not, though perhaps it should be, a central concern of students of Classical Antiquity. It employs the Harvard system of reference, which the Editor was urged to adopt (*LCM* 8.6[Jun.1983], 81), or at least to encourage contributors to employ, for he is so unwilling to compel them to anything, but rather to indulge them, that he believes he would print an article boustrophedon if he were asked to - perhaps a rash statement which some one will take up!

He has commented before on a tendency, or perhaps a temptation, in our studies, to devote resources to those areas which are more easily studied by those with less grounding in the ancient languages than has in the past been the case in this country - that is to Studies, to subject matter and ideas and history and art and archaeology - and to encourage their illumination by new techniques of study developed in other more modern disciplines. A phrase much heard in beleaguered Departments, wondering what new economies are going to be forced upon their Universities in an ominously numbered New Year, is 'Commitment to linguistic Classics' - to some of us a rallying cry, believing as we do that all study of another culture, however exciting may be 'new approaches', must be firmly based on a knowledge of the language of that culture, which can exercise a necessary control upon wilder speculations, but to others, we fear, sometimes a war cry, carrying as it does some critical overtones which suggest that 'linguistic' Classics denies itself the more important and attractive areas of a study of which it is only a part.

This message seems to be so obscure as to have been 'coded', as political statements increasingly are in this country. But the Editor remains confident that readers are skilled at interpreting his more Delphic utterances. One indeed said that it was good to know that *LCM* is safe still - though the Editor never seriously believed that Dr Blumenthal was ever likely to forbid him to continue with its publication - or indeed that he was ever really likely to obey any such order! What really has made its position much more secure is miniaturization, which has had a remarkable effect on the accounts for 1982, which the Editor has only just been able to complete after much urging by the Inland Revenue, who have an interest in what is a tax-paying private business. No increase in the subscription is in prospect.

Another, in renewing his subscription, provided another of those testimonials which help to reconcile the Editor to his labours and which he therefore reproduces: '*LCM* continues, as always, to be both stimulating and informative. There is always at least one article per issue which I must read, either for amusement (etymological sense or figurative) or for my own work. I note that *LCM* has inevitably begun to appear in footnotes of standard texts, a sure sign of success'. That is what the Editor is always chary of claiming, being content to survive, hopefully also through 1984 and into double figures. But he must warn readers that he is less confident this year that 9.1(Jan. 1984) will be in the hands at least of inland subscribers before the great Christmas shut-down, as there are vast arrears of business correspondence to catch up with, for which he apologizes to all those who should have received answers before now. So they may have to be content with this number for their Christmas amusement.

\* This is an error: Stir-up Sunday is the Sunday next before Advent and the Editor is a week out.

AMELIE KUHRT (University College, London): *A brief guide to some recent work on the Achaemenid Empire*

*This represents the text, scarcely altered, of a paper given at the Norman Baynes 'Weekend' in Liverpool on 22.9.1982. The Editor must apologize to author and readers that the publication has been delayed longer than perhaps it should have been. As it is, it should make good Christmas reading.*

Before the systematic excavation of the Middle East began in the early part of the nineteenth century and resulted in the decipherment of Old Persian, Mesopotamian and Egyptian texts, knowledge of the Old Persian Empire was limited to the information contained in classical writers and in the Old Testament. Both types of traditions presented Persian rulers or used Persian settings primarily as contrasts to their own cultural values. This element has recently been much emphasized in relation to Herodotos in the work of Alan Lloyd (1975-6), Waters' work on Herodotos on oriental despots (1971), and most recently in Heleen Sancisi-Weerdenberg's doctoral dissertation (1980); though it should be noticed that the last-mentioned work attempts to disengage some genuine Persian traditions from their Greek dress. In spite of these limitations one should recognize that Herodotos still provides an honest account of the Persian empire and its institutions, as Momigliano (1979) has emphasized, and thus constitutes still one of the most important sources. In many cases one must, however, be aware of the problems that existed for a Greek in understanding Persian political concepts, as E. Will (1972) has pointed out very clearly in relation to the famous constitutional debate in Herodotus 3.80-82. The 'hellenization' of Persian themes and personalities can be most clearly seen, of course, in Aeschylus' *Persai* - a subject recently treated by Vogt (1972) -, as well as in Xenophon's *Kyropaideia*, which is best regarded as a moralistic novel in which the founder of the Persian empire displays all the virtues appropriate to Xenophon's concept of the ideal ruler.

Ktesias, the author of the 'lost' *Persika*, appears to have provided very little useful information on the functioning of the Persian empire, although Momigliano (1969) suggested some time ago that he might provide a valuable source for the historical traditions current within Persia itself. Drews (in an article in 1965 and in his book in 1973) has attempted to demonstrate that Ktesias certainly preserved authentic Mesopotamian traditions: if one accepts his conclusions little would stand in the way of regarding Ktesias as a valuable source for how the Persians viewed their own early history. As far as I am aware, however, a study of this aspect of Ktesias is outstanding at present, and the attempt to wrest useful information from Ktesias' 'peepshow' style account of Persian court-life is a somewhat hazardous undertaking, although the work was clearly a great success with Greek, Roman and Byzantine audiences.

The book of Esther also uses a Persian court setting, but its partisan character is plain, and the characterization of life in Persia comparable to that found in classical authors (pointed out also in relation to its literary form by Momigliano [1977]): the Persian ruler is a despot, a victim of court and harem intrigues, a puppet manipulated by his advisers (cf. generally Bickerman 1967). The character of Persian rulers as portrayed primarily in Ezra-Nehemiah is of a strikingly different type, and provides the basic (albeit problematical) evidence for Persian rule in Judah. From this stems the widespread reputation of the Achaemenids for religious tolerance; a fairly up-to-date guide for further reading on the immensely complex question of the texts is G. Widengren (1979).

Apart from this material, little else was known of the Achaemenids until the process of decipherment by Grotefend, Rawlinson and others in the early nineteenth century made the Old Persian inscriptions available, led to the eventual decipherment of material in Akkadian and Elamite, and encouraged further excavations in the Middle East. Concurrent with this was the decipherment of Egyptian and the extensive excavation of Egyptian sites. Slightly earlier, study of the Parsee community of India and of their religious book, the Avesta, had drawn attention to the monotheistic and moral teachings of Zoroaster, and the language of the Avesta itself helped in the decipherment and understanding of Old Persian by providing examples of a more archaic form of a Persian-related language which also appeared to retain echoes (at least) of the original social structure of the Iranian tribes. Some guidance in this complex field of study is provided by the relevant volume of the *Handbuch der Orientalistik* (1968), and by Frye (1962). The general view of a clear separation between the Median and Old Persian forms of Iranian, thus making it possible to detect extensive institutional borrowings by the Achaemenids from their Median predecessors, has recently been challenged by Lecoq (1974), who argues for the existence of a Medo-Persian *koiné*. The acceptance of this suggestion would throw major doubts on the presumed nature of the Median-Old Persian relationship.

In the early sixties the prevailing theory on the Iranian migrations was that presented by Ghirshman (1963), according to which the movement of Iranian tribes could be traced from the Caucasus into Iran; this appeared viable on the basis of Assyrian texts and archaeological remains. It has been seriously challenged by Cuyler-Young (1967), who argued convincingly for a gradual movement of cattle-herders from the east of the Caspian; these eventually coalesced with local principalities in the Zagros, resulting in the emergence of Median rulers in some centres in the eighth century B.C.. This theory received support from Levine (1969), who has made a strong case for limiting the area of Median presence in the Zagros contemporary with the Assyrian empire to the Kermanshah-Hamadan region<sup>1</sup>. As a result the sites of Nush-i Jan, Godin Tepe and Baba Jan are now commonly termed 'Median' sites, each consisting of a small, fortified citadel representing the seat

1. Not everyone is in agreement with Levine's arguments: cf., for example, J. Reade, 'Hasanlu, Gilzanu and Related Considerations', *Archäologische Mitteilungen aus Iran* 12, 1979, 175-181.

of a Median clan-chief. One problem with such an identification is that nothing fundamentally distinguishes these centres from other contemporary sites further north in the Urmia-basin which can be assigned firmly to the local Mannaeans population. This has, in fact, led to a very interesting thesis being propounded recently by Helm (1981); according to this various leaders in the Zagros area were included at a later date in national hero-tales to form a seemingly continuous story of the early history and achievements of the peoples of the region; related to this aim was the fact that they were linked together dynastically, although in fact the exploits concerned all sorts of local peoples who later formed part of what became the central Median area. This hypothesis at a stroke would eliminate the problematical Deiokes and Phraortes as early Median kings, and point to a possible explanation for the difficulty in specifying 'Irano-Median' elements on the Zagros sites.

The Cuyler-Young thesis, Levine's geographical studies, and the now well-established identification of Malyān in Fars (Persis) as the site of Anshan, have also influenced notions concerning the early history of the Persians. In 1974 Stronach put forward very attractive arguments to demonstrate that the Persian tribes most probably entered Fars directly from the Iranian plateau, and that the name Parsumaš/Parsua/Parsā was subsequently adopted as a tribal name for the area, although it continued to be used alongside the traditional term 'Anshan' well into the reign of Cyrus<sup>2</sup>. This eliminates completely an earlier, elaborate hypothesis of a dual line of kings (one ruling Anshan, one ruling Persia), which had received support from a possible translation of a passage in Darius the Great's Behistun inscription; a telling and seemingly conclusive blow to the dual kingship theory was administered by E.Voigtlander (1978) with her publication of the Babylonian version of the Behistun text: the Babylonian rendering of the disputed passage states clearly 'nine kings of an eternal lineage are we'. As Darius the Great's grandfather and great-grandfather's inscriptions on gold tablets have been regarded by a majority of Old Persian specialists as fifth century fakes, on epigraphical grounds, the disappearance of the junior royal house need not cause too much consternation<sup>3</sup>.

In order to follow the process of the formation of the empire under Cyrus II and Cambyses one is completely dependent on sources external to Iran: the Old Persian texts that have been attributed to Cyrus at Pasargadae are now with good reason regarded by many scholars as later additions to Cyrus' buildings by Darius I (Hinz and Borger, 1959; Hinz 1972). Thus, apart from the framework provided by Herodotos' account, the only contemporary sources for the Persian conquest are Babylonian and Egyptian. The Babylonian ones consist mainly of large numbers of economic and legal documents which help to fix the chronology of Cyrus' conquest and demonstrate the virtual lack of change in the administrative machinery during his and Cambyses' reign<sup>4</sup>. Apart from these there are three extremely interesting texts that relate specifically to the Persian conquest: first, the so-called Nabonidus Chronicle (most recently treated by Grayson 1975), which covers most of the reign of the last Babylonian king and the accession and first regnal year (partially) of Cyrus. It forms part of the Babylonian Chronicle series, a type of Babylonian historiography based on annual records and remarkably objective, so that the main limitations of the text are its fragmentary state of preservation (the last column, which would have covered more of Cyrus' reign, is almost completely lost). This rather dry and, at crucial points, problematical account of Cyrus' capture of Babylon is amplified by the 'Persian Verse Account of Nabonidus', a unique propaganda text not worked on fully since its publication by Smith in 1924<sup>5</sup>, although a more up-to-date translation can be found in the volume of *Ancient Near Eastern Texts* (Oppenheim 1969). It vilifies the reign of Nabonidus to the point of caricature, and extols that of Cyrus by contrast; it appears to have been composed by scribes associated with the Marduk cult in Babylon. Much better known and also better preserved is the Cyrus Cylinder, a text now shown by Harmatta (1971), Walker (1972) and Berger (1975) to be closely linked stylistically to Assyrian royal building texts, particularly those of Assurbanipal, the last great Assyrian king, who restored Babylon extensively. This appears to suggest that the tradition to which Cyrus linked himself was modelled on that of a benevolent Assyrian emperor rather than the indigenous Babylonian dynasty.

The Egyptian material is less ample, but still sufficient to counter the standard picture presented by Herodotos, and to demonstrate that the invidious contrast usually drawn between Cyrus and Cambyses is mere proof of the effectiveness of Egyptian nationalist propaganda, the persistence of which has been interestingly discussed recently by Lloyd 1982. The Serapeum stele and Apis bull coffin dedicated by Cambyses have received no full treatment since Posener (1936), but the

2. R.Ghirshman revised his position on the Iranian movements in 1977, and argued for a two-pronged migration (*L'Iran et la migration des Indo-Aryens*, Leiden).
3. The arguments are conveniently summed up by M.Dandamayev, 'The dynasty of the Achaemenids in the early period', *Act. ant. Scient. acad. Hung.* 35(1977), 39-42.
4. The continuity in terms of legal and economic institutions is apparent from a number of studies of Neo-Babylonian texts (esp. Uruk material), which frequently cover the period to 520 B.C.; e.g. D.Cocquerillat 1968, *Palmeries et cultures de l'Eanna d'Uruk (559-520)*, ADFG Band 8, Berlin; H.M.Kümmel 1979, *Familie, Beruf und Amt im spätbabylonischen Uruk*, ADOG 20, Berlin - to name but two.
5. S.Smith 1924, *Babylonian Historical Texts relating to the capture and downfall of Babylon*, London.

statue of Udjahorresne which shows an Egyptian official happily accepting Cambyses as legitimate successor to the Saite dynasty is now easily accessible in a new translation by Lichtheim (1980)<sup>6</sup>. The most succinct treatment of the history and condition of Egypt as a satrapy remains Bresciani 1969.

As has already been pointed out, there is no undisputed inscriptional material of these rulers from Iran itself, but the character of Cyrus' capital at Pasargadae has been extensively investigated by Stronach, and the results published by him in 1978. A tomb not unlike that of Cyrus as well as the remnants of what may be a palace have been identified near Persepolis and described by Tilia (1974a; 1974b), and some scholars have suggested that it should be attributed to Cambyses; if correct this would have interesting implications for the development of Persian kingship in his reign - but the evidence remains ambiguous.

The formation of the empire in the case of both rulers is characterized by a conquest-policy which substituted Persian hegemony for local government, while administrative reorganization appeared to be almost totally lacking<sup>7</sup>; political units were added wholesale, and united only in the person of the Persian king, who adopted the indigenous royal titles, insignia and dress; immediate predecessors were discredited in order to legitimize the Persians' right to rule, and primarily local people used as administrators; some lightening of fiscal and labour obligations as well as resettlement of small groups of deported populations may be seen as attempts to establish a favourable attitude to the conquerors<sup>8</sup>.

The formative stage of the Persian Empire provides the background against which the Bardiya (= Smerdis) rebellion and Darius' seizure of power have to be assessed: this constitutes a turning point in the history of the empire, and with Darius I royal inscriptions and administrative texts become available for the first time in Iran itself, and the archaeological material from the area, too, becomes far fuller. The most interesting, and indeed major study of the Bardiya revolt is that of Dandamayev published in Russian in 1963 and now available in German in a revised edition (1976). After emphasizing the general incredibility of the 'false Smerdis' story as retailed by Darius at Behistun and elaborated by Herodotos, and drawing attention to the fact that the general revolt which took place in the empire was directed only against Darius' seizure of the throne and not against Bardiya, his thesis is to demonstrate a conflict within the Persian central area itself; this, he concludes, arose between the clan-nobility who provided troops and commanders for the conquests but whose powers were not growing in direct ratio to the Persian expansion: all the credit and increased wealth was making the king, local dignitaries in conquered countries, and the Persian population, more powerful, while the status of the nobles showed no real increase. Bardiya's revolt consisted in taking the Cyrus-Cambyses policy one step further, in that he tried to abolish the nobles' privileges and economic power bases altogether; this led to his assassination by the seven conspirators, Darius emerged as king, and as a result the mass of the population rebelled. What Dandamayev particularly emphasizes is that, while one might well interpret revolts in conquered countries as simply nationalist attempts to regain independence at a time of dynastic confusion in Persia, the fact that Persia itself was in revolt for quite a long time under the leadership of a man who claimed to be Cambyses' brother Bardiya must reflect an internal Persian social conflict which had developed as the result of the acquisition of an empire.

One of the outcomes of this conflict, according to the arguments of this stimulating study, was the appearance of the Old Persian cuneiform script from Darius onward; this is to be interpreted as a reaction to Cyrus' and Cambyses' policy of respect for other cultures, and their exclusive use of foreign languages for their own administrative needs and proclamations. The appearance of the Old Persian script is then to be taken as a conscious invention designed to provide the Persians with their own and unique form of writing. This would explain too why the use of the Old Persian script declined both quantitatively and qualitatively, certainly after Xerxes' reign. Dandamayev is not alone in arguing for a conscious invention of the script by Darius - a large number of scholars would now accept it, especially as royal inscriptions clearly datable earlier than Darius I have consistently failed to turn up; for a good discussion of this question see Lecoq 1974b. The total number of royal inscriptions written in Old Persian is small - virtually all are contained in Kent (1953), and only four, relatively important ones, have turned up since then: three of Darius and one of Xerxes (Hinz 1972; Perrot et al. 1974). Out of an approximate total of one hundred Old Persian inscriptions about sixty are attributable to Darius I and twenty to Xerxes, leaving a further twenty or so, very short texts, to divide among the kings of the subsequent hundred and thirty-five years. A further important point to note is that nearly all Old Persian texts are, first, either tri-lingual or quadri-lingual, and, secondly, have nearly all been found within the central area of the Persian empire: Fars, Elam, Media. Exceptions to this are almost always brief texts on small portable objects such as seals and vases. Thirdly, virtually all Old Persian inscriptions are royal - private ones appear to have been non-existent.

6. The text has received an extensive treatment by A.B.Lloyd 1982, 'The Inscription of Udjahorresne, a Collaborator's Testament', *JEA* 68, 166-180.
7. The sparse evidence for the existence of 'feudal' holdings already under Cambyses in Babylonia is presented in M.Dandamayev 1967, 'Die Lehnbeziehungen in Babylonien unter den ersten Achämeniden', *Festschrift ... W.Eilers*, Wiesbaden, 37-42.
8. Achaemenid policy in relation to deportees is actually a knotty problem; cf. A.Kuhrt 1983, 'The Cyrus Cylinder and Achaemenid imperial policy', *JSOT*

Clearly, then, the script was not used for administration, although it is plain from the Elamite texts from Persepolis, and from the Aramaic material (mainly from fifth century Egypt), that the Old Persian language must have been used for administrative directives. Indeed, Gershevitch (1971; 1979) has argued that 'Imperial Elamite' was most probably an archival language at this period. The number of Elamite texts from Persepolis is enormous, and to the hundred and thirty-nine Treasury texts published by Cameron (1948; 1958; 1965) have been added the two thousand and eighty-seven Fortification texts published by Hallock (1969), although the number published far from exhausts the available texts. Hallock (1971), Hinz (1971) and Lewis (1976 and 1980) have demonstrated the usefulness of this material for gaining some insight into the functioning of royal administration in the area of Fars and Susa; but the sheer linguistic problems of these texts are enormous, so that Koch's (1977) attempt to investigate the administration of cults, and religious conditions, on their basis is so marred by doubtful reconstructions of Old Persian terms that it makes her potentially useful study almost impossible to use. The main work being done on the Elamite language of this period at present is conducted by Vallat and Grillot in Paris. An interesting addition to this corpus is the find of an as yet unpublished fragmentary Elamite text at Kandahar by Svend Helms in 1978.

To what extent Aramaic was used concurrently with Elamite in Persia is still not fully agreed among scholars, but its widespread use in the empire is well established. The main corpus of material comes from Egypt, and a large selection of this has now been made available in translation by Grelot (1972); a survey of all the available material is contained in Greenfield's (1976) article. More Aramaic texts have been found at Samaria (Cross 1963), though they are still unpublished, as well as at Sardis, Daskyleion and, of course, on the Xanthos stele, now fully published in *Fouilles de Xanthos* (1979) - the Aramaic text has also been discussed by Teixidor (1978). The stone mortars and pestles from Persepolis, with Aramaic ink inscriptions, were published by Bowman (1970), but his interpretation of these as ritual texts has been much questioned, and generally rejected. A recent and clear presentation of the difficulties in accepting Bowman's view can be found in Boyce (1982), which is her second volume of the *History of Zoroastrianism*, and covers the Achaemenid period. In this she argues strongly in favour of Cyrus II as a practising Zoroastrian, suggests that especially a number of Mesopotamian deities strongly influenced the shaping of certain Iranian deities, demonstrates the absence of a highly differentiated priesthood, although the magi certainly constituted distinct cultic personnel whenever they can be identified, and traces the development of a temple and statue cult under Artaxerxes II.

A major restriction in knowledge of the Persian empire is the small number of excavated Persian sites: Pasargadae, Persepolis and Susa are exceptions rather than the rule, and problems abound, particularly concerning the last two. An enormous amount of work was done at Persepolis by Tilia (1968; 1972/1978; 1977), which has clarified settlement around the terrace, the fortified character of the terrace itself, the arrangement and dating of reliefs, and the architectural history of palace H. Farkas' (1974) study of Achaemenid sculpture, and Nylander's (1970) of the development of architectural techniques on Achaemenid sites have provided data that has already become fundamental to anyone interested in the art of the empire. The precise function of buildings on the Persepolis terrace still remains disputed, although a number of misapprehensions, particularly concerning the New Year festival, have been cleared up by Nylander (1972 and 1974). Roaf's (1978) thesis on the methods of sculpting the Persepolis reliefs has cast an interesting light on the organization of the gangs of workmen employed there.

The stratigraphy of Susa is unfortunately in a lamentable state, but work has begun to appear in the *Cd'AFI* series to remedy this state of affairs. Undoubtedly one of the most important finds made there in recent years is the discovery in 1972 of a colossal (headless) statue of Darius I *in situ*: the statue was probably made by Egyptians, and is the only extant sculpture of a Persian ruler in the round (Perrot et al. 1974). A major help for keeping track of Iranian archaeology, it should be noted, is now available: Vanden Berghe's (1979) valuable bibliography on the archaeology of Iran, which has already been updated in a supplementary fascicle (Vanden Berghe and Haerinck 1981).

Asia Minor suffers from a lack of publication of scattered material, although Shahbazi (1975) has gathered a good deal of the Lycian material together, and Hornblower's (1982) monumental study of Caria now fills part of the gap more than adequately. Syria-Palestine are under-represented in terms of Old Persian archaeological remains; Moorey's (1975 and 1980) publication of the cemetery at Deve Höyük near Carchemish, containing the armour of perhaps Scythian soldiers, presumably part of an Achaemenid garrison post in the area, is an almost isolated example of real archaeological information for the North Syrian area. The long-announced study by Stern (1982) of the material culture of Palestine in the Persian period has at last appeared, drawing a vast amount of very dispersed information on this area together<sup>9</sup>. Egyptian art, which displays a remarkable development (for the first time) in the direction of true portraiture in the Persian period, has been fairly fully documented by Bothmer (1961), and the material culture of Babylonia in the same period was the subject of Haerinck's thesis, of which, unfortunately, only his work on the Persian palace at Babylon (Haerinck 1973) is available. A number of sites in Eastern

9. A very interesting collection of bullae and seal impressions of what was probably an official archive dating to the Persian period has been published and discussed by N. Avigad 1976, *Bullae and Seals from a Post-Exilic Judaean Archive*, Qedem 4 Jerusalem.

Iran, Afghanistan and Soviet Central Asia, with Achaemenid levels, have been excavated although never fully published; interesting work on the interrelations of Scythian, Bactrian and Achaemenid art and archaeological development in the North Eastern area generally have been carried out both by Soviet and French scholars (Cattenat and Gardin 1977; Kuzmina 1977).

Some of the details of the functioning of the empire have received attention in two careful studies of specific areas using the evidence of internally coherent archives. For Egypt this has been done by Porten (1968), using the evidence of the Jewish garrison colony at Elephantine, which provides a clearer picture both of the organization of such a garrison, its relations to the superior commander at Syene, and those with the local Egyptian peasants - although many questions concerning the chain of authority between colonists-garrison commander-nomarch-satrap remain problematical. In the case of Babylon Stolper (1974) has published further texts from the Murašû archive and subjected the material to a new study. On the basis of this he has proposed the existence of a census-system for both fiscal and military dues, and the direct involvement of the Achaemenid government in land-holding in Babylonia both through its ownership of major elements of the irrigation system as well as through the large estates belonging to members of the Achaemenid royal family, which provided not only a support for the management of the empire, but also for the political activities of their owners.

As can be seen from this very brief outline, the sources for the empire are limited:- classical writers with necessarily circumscribed or parochial interests, the Old Testament material reflecting the immediate concerns of the Jerusalem community, a handful of Old Persian royal inscriptions; the number of Elamite texts, albeit large, come (with one unpublished exception) only from Persepolis, are related exclusively to royal administration, their geographical horizon just extends beyond Fars and they are limited to the years 509-458. Apart from that, the Aramaic texts from Egypt cover the last half of the fifth century, and the Babylonian texts, too, become distinctly sparse after the reign of Darius II; nor is the archaeological material, except from three sites in Iran, very plentiful. At present there seems no particular prospect for this source-profile to change dramatically - more material of a similar type may be found or published, but major problems will not be solved by that.

Three relatively recent pieces of work might just briefly be cited here to indicate some different approaches that have been attempted to surmount some of these obstacles and cast fresh light on the Achaemenid imperial structure. Herrenschildt (1976) in her dissertation on the literary form of the Old Persian royal inscriptions (not yet published, but some of her conclusions have been presented in articles) has attempted to isolate specific political concepts; thus, in her view, the 'empire' was conceived by the Persians as 'this earth', the fringes of which were defined by peoples on the frontier not actually subject to the Persian king but perhaps conceived as 'allies' (Nubians, Arabs, Scythians). The Persian king, therefore, was ruler over the peoples/countries making up 'this earth', and it is these *ēšm* which are contained in the so-called 'satrapy-lists', the important rider being that the lists cannot possibly represent administrative districts. Root (1979) has analysed particular motifs in Old Persian imperial art and the manner in which they have been reshaped; from this she concludes that the Achaemenid kings, while certainly borrowing virtually all their repertoire of motifs from Assyria and Egypt, consciously rejected the brutal elements inherent in many of them because they were incompatible with their own vision of kingship; subject peoples are thus shown deliberately as co-operative allies rather than subjugated foes. In this way the Achaemenids created a truly innovative and unique imperial art that reflected their specific ideal of kingship. Briant (1982) concentrates on those peoples both within and at the edges of the empire whose mode of subsistence (pastoralist or nomadic) made it impossible for them to be subsumed into a monolithic imperial administrative system (if such a one indeed existed) nor amenable to normal diplomatic procedures. By demonstrating the interdependence of the central state and these peoples, and using the evidence of classical sources on Alexander and classical geographers, he makes a good case for the practice of 'gift-relationships' as a major means used by the Old Persian kings to 'manage' successfully their relations with these population groups. The book, in fact, helps to demonstrate the remarkable elasticity of the empire, a characteristic which is shared with the Mauryan empire, a useful analysis of whose functioning (for comparative purposes) one can find in the article by Thapar (1982)<sup>10</sup>.

10. Just at the point of sending off this article a new and very useful book has appeared: J.M. Cook 1983, *The Persian Empire*, London; this provides an extremely useful and naturally more expanded account, taking, in particular, the Greek historiographic material into account.

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## List of less familiar abbreviations

BA	<i>Biblical Archaeologist</i>
CdDAFI	<i>Cahiers de la Délégation Archéologique Française en Iran</i>
E&W	<i>East and West</i>
IsMEO	<i>Istituto italiano per il Medio ed Estremo Oriente</i>
JEA	<i>Journal of Egyptian Archaeology</i>
JNES	<i>Journal of Near Eastern Studies</i>
RA	<i>Revue d'assyriologie et archéologie orientale</i>
TPS	<i>Transactions of the Philological Society</i>
ZA	<i>Zeitschrift für Assyriologie</i>
ZDMG	<i>Zeitschrift der Deutschen Morgenland Gesellschaft</i>

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YVAN NADEAU(Edinburgh, Faculty of Arts Office): *Who traduced Juvenal?* (3.109-113 & 6.185-191, 14.256-264, 8.121-124, 8.100-112, 4.150-154)  
 LCM 8.10(Dec.1983), 153-157

## A. GREEK SECRETS

1. Consider the following passage. Juvenal, in his third satire, has just been talking about how skilful the Greeks are at ingratiating themselves with their patrons; with a mere *praeterea* serving as link, he launches into an apparently unconnected theme:

*praeterea sanctum nihil taut ab inguine tutum,  
 non matrona laris, non filia uirgo, nec ipse 110  
 sponsus levis adhuc, non filius ante pudicus.  
 horum si nihil est, auam resupinat amici.  
 [scire uolunt secreta domus atque inde timeri] 113*

113 is deleted by Clausen in his OCT, in the Rudd/Courtney edition of *Satires* 1, 3, 10, in Ferguson's edition, and its deletion is accepted by Courtney in his commentary, in which he says that it is 'made up out of 52 (*secreti*) and 57 (*timearis*)'.

2. If 113 is read out of context, we have a common situation: that of the client who has knowledge of his patron's guilty secrets and is therefore a potential *delator*. That is the situation in lines 52-57 of this satire, referred to by Courtney, and in e.g. 1.33 *magni delator amici*. That situation seems unconnected with the present context.

3. In the present context, after the enumeration of the sexual misdemeanours of 110-112, *secreta* acquires an extra connotation: the sexual sense of *secreta*. That sense is exemplified in the OLD by Gellius 9.10, where are being discussed the discretion and reserve shown by Virgil in his description of the sexual exchange of Venus and Vulcan in A.8.404:

*sed puris honestisque uerbis uenerandum illud concubii pudici secretum neminem quemquam alium dixisse.*

A double sense of *secretum* is exploited by Petronius in 112, in the story of the Widow of Ephesus, where *delectatus miles et forma mulieris et secreto*, and, as P.G.Walsh (*The Roman Novel*, p.91) indicates, in Petronius 17, where hardly a thousand men are said to have known the *secreta* of Quartilla, *tot annorum secreta quae uix mille homines nouerunt*.

4. An ambiguous use of *secretum* is found in more than half the passages of Juvenal where the noun or the adjective occurs. I quote only 6.314-318

*nota bonae secreta deae, cum tibia lumbos 314  
 incitat ...  
 o quantus tunc illis mentibus ardor 317*

and 6.402-404

*concubitus  
 haec eadem nouit quid toto fiat in orbe,  
 quid Seres, quid Thraces agant, secreta nouercae*

*et pueri, quis amet quis diripiatur adulter.* 6.404

5. Finally, after comparing 6.189-191 with all the above passages, but especially with the Gellius and with 6.318, I would say that in 6.189-191,

*hoc sermone [in Greek] pauent, hoc iram gaudia curas,*

*hoc cuncta effundunt animi secreta. quid ultra?*

*concupiunt Graece,*

the point of the concluding *sententia* is sharp only if we see that *secreta*, as in the passage of satire 3 we are studying, has two senses: a straightforward one, 'the secrets of the heart' if we stop at *secreta*; but acquiring an ambiguity as we move on to *concupiunt* - the unexpected gradation being from *animi secreta* to *secreta concubitus*. The verbal collocation *secreta / concubitus* is common to all three passages, the Gellius, Juvenal 6.318 and 6.191.

6. Looking back upon 3.109-113, my view is that, if only we consider 113 as written in jest, as providing a punning *sententia* to the passage, it is an understandable line - even perhaps amusing. If we now look at 109-113 in its context, we see that those lines lead the satirist, with the utmost economy, from one theme, the ingratiating skills of the Greeks (exemplified in the preceding passage), through the connected theme of their skills as seducers (exemplified in the present passage), to the theme of their skills as *delatores* (as shown in what follows, the story of Barea Soranus betrayed by his Greek client). *secreta* is the exact point of transition, since it looks back upon sexual *secreta* and forward upon more generalized *secreta*.

7. As we are talking about Greeks and secrets, let us go back to a passage of Juvenal 6 we have touched upon, and widen our view to reveal more square brackets in Clausen's text, at 6.185-9:

*nam quid rancidius quam quod se non putat ulla*

*formosam nisi quae de Tusca Graecula facta est*

*de Sulmonensi mera Cecropis? omnia Graece:*

[*cum sit turpe magis nostris nescire Latine.*] 188

*hoc sermone ... etc..*

8. The point Juvenal is making, and on which he is basing two jokes in lines 185-191, is that it ill befits (*sit turpe*) Roman matrons to make love Greek style. That is, Greek women and courtesans may move their bodies lasciviously in love-making, but Roman matrons ought to do it unresponsively and motionless. I have illustrated the point in LCM 7.5 (May 1982), 69, by quoting Lucretius 4.1263-1277

*et quibus ipsa modis tractetur blanda uoluptas*

*id quoque permagni refert ...* 1264

*nec molles opus sunt motus uxoribus hilum.* 1268

*nam mulier prohibet se concipere atque repugnat,*

*clunibus ipsa uiri Venerem si laeta retractat* 1270

*atque exossato ciet omni pectore fluctus;*

.....

*idque sua causa consuerunt scorta moueri* 1274

*ne complerentur crebro grauidaeque iacerent*

*et simul ipsa uiris Venus ut concinnior esset;*

*coniugibus quod nil nostris opus esse uidetur.*

9. Line 188 above, bracketed by most modern editors, has a double meaning. The reader thinks that Juvenal is talking about Greek speech, in that the line seems to refer to a common discussion of grammarians, i.e. to what extent their pupils should be taught Greek before Latin (cf. Quintilian 1.1.12-13), and the avoidance of barbarisms so as to achieve good Latinity (cf. Cicero, *de oratore* 3.40-44, *Brutus* 140: *Rhetorica ad Herennium* 4.17). But it quickly dawns on him that the satirist is joking. He is not referring to Latin or Greek speech only; he is saying that it is *turpe* (we would say 'degenerate') that Roman women cannot make love Latin-style, i.e. with due decorum, but must do it Greek-style, i.e. like whores. The double meaning is the point. The joke leads to a second joke on the same theme in *secreta animi / concupiunt Graece* (as we have seen above in 6). A good joke bears repeating.

## B. THE WORK AND PLAYS

L. Consider this passage, 14.256-264:

*monstro uoluptatem egregiam, cui nulla theatra,*

*nulla aequare queas praetoris pulpita lauti,*

*si spectes quanto capitis discrimine constant*

*incrementa domus, aerata multus in arca*

*fiscus et ad uigilem ponendi Castora nummi,*

*et quo Mars ultor galeam quoque perdidit et res*

*non potuit seruare suas. ergo omnia Florae*

*et Cereis licet et Cybeles aulaea relinquant:*

*tanto maiores humana negotia ludi.*

we start from the concluding epigram: 'so much finer are the games of human life', says the Loeb translator, and the Penguin's, 'Mankind's commercial dealings offer far bigger sport', and Courtney and Ferguson in their commentaries refer to Shakespeare's 'all the world's a stage'. The parallels adduced by Courtney and Mayor show that they see that there are two ideas in Juvenal's mind, 1) the idea that life is as much worth watching as a play, and 2) that Fortune makes sport of man's life. Cf. Horace, *Ep.* 2.1.194-198

*si foret in terris, rideret Democritus seu*

*diuersum confusa genus panthera camelo  
siue elephas albus uulgi conuerteret ora;  
spectaret populum ludis attentius ipsis,  
ut sibi praebentem nimio spectacula plura,*

and Tacitus, *Annals* 3.18

*mihi, quanto plura recentium seu ueterum reuoluo, tanto magis ludibria  
rerum mortalium cunctis in negotiis obuersantur.*

2. But in the Juvenal the ideas are less interesting than the verbal manipulation. *negotia* and *ludi* are antithetical, when they bear their first and most obvious meaning, the one set out first in the *OLD*, 'sport, play' for *ludus*, 'work, business' for *negotium*. It is the coming together, at the end of the passage, of those two antithetical meanings that makes the *sententia* striking, with its suggestion that business is play, work is amusement.

3. But it is a different sense of each word that is 'prepared for' in the preceding passage: for *negotium* the sense prepared for, by lines 259-260, is the sense of 'commercial transactions'; and for *ludi* the sense prepared for, by lines 256-258 and 262-263, is 'stage plays', *ludi scaenici*. If we were to translate the concluding *sententia* in the sense 'prepared for', it would be 'men's commercial dealings make better plays'; if we translate it in the sense called for by the antithesis, separating the epigram from its context, the sense called for by the interplay of the word *negotia* with the word *ludi* would be 'men's work is play'. Somewhere in the background is a sense like that to be found in the passage of Tacitus quoted above.

4. The real usefulness of the passages of Tacitus and Horace quoted above is not only their similarity to the Juvenal, but their difference also. While Juvenal's concluding *sententia* has all the resonance of the Tacitus, the build-up leading to the *sententia* works with deliberate and rhetorical precision towards the two different shades of meaning of the two key-words, *negotia* and *ludi* - thereby making the reader do a 'double-take' when he comes to the conclusion.

5. The tone of the passage, its rather obvious rhetorical *congeries* interrupted by a digression, should alert readers to Juvenal's less-than-serious purpose. It is a verbal climax that we come to, the interest of which is in the Protean shift of meaning of words. Three strands of meaning come together: *humana negotia ludibria sunt* [*Fortunae*], the 'Tacitean' sense - the Loeb translation comes close to that sense; *humana negotia, pecuniae adipiscendae dedita, meliora spectacula praebent quam ludi scaenici* - the Penguin translation comes closest to that sense; *quod magno negotio fit, ludus et facile est* - which is the punning paradox.

#### C. SPOILS FROM THE VANQUISHED

1. Consider another passage, 8.121-124, where Juvenal is urging Roman nobles to be careful which of their provinces they plunder:

*curandum in primis ne magna iniuria fiat  
fortibus et miseris. tollas licet omne quod usquam est  
auri et argenti, scutum gladiumque relinques.*

[*et iaculum et galeam; spoliatis arma supersunt*]

So the text appears in Clausen's *OCT*. It is translated by the Loeb:

You may take from them all the gold and silver that they have; you will still leave them their shields, their swords, their javelins and helmets; plundered though they be, they will still have their arms.

and by the Penguin:

You may strip them of all  
Their gold and silver, they still possess sword and shields,  
Helmets and javelins: the plundered keep their weapons.

Courtney's conclusion on the textual problem is:

The difficulty lies in *et iaculum et galeam*. Though I agree that Juvenal should not have written the words, ... [I] incline to think the text sound.

2. The general context of the passage, in which Juvenal is talking about the plundering of provinces by Roman nobles, leads to the translation of *spoliatis* in the last line as 'plundered'. But no Roman would need to be reminded that a common sense of *spoliare* (sense 2 in the *OLD*) is 'to strip a defeated enemy of arms, equipment etc...'.  
3. It is that sense of *spoliare* which is prepared for by the immediately preceding *congeries* of *scutum*, *gladium*, *iaculum* and *galeam*. In other words, Juvenal is playing on two senses of *spoliare*: *spoliati* means 'plundered', but the effectiveness of the *sententia* consists in its also meaning 'stripped of arms', the effect of the *sententia* residing in the verbal paradox that *spoliati*, who should have no *arma*, still have their *arma*. The apparent artistic lapse of the verbose enumeration is deliberately unsubtle rhetoric pointing to the joke to come.

#### D. WHEN ARE THE BIGGEST NOT THE LARGEST?

1. Juvenal is comparing, in 8.100-112, the glorious past, when despoiling the provincials was a productive business for Roman governors, with the present state of affairs, when the provincials have nothing left worth stealing:

*plena domus tunc omnis, et ingens stabat acervus 100  
nummorum, Spartana chlamys, conchyliis Coa  
et cum Parrhasii tabulis signisque Myronis  
Phidiaeum uiuebat ebur, nec non Polycliti  
multus ubique labor, rarae sine Mentore mensae.*

inde †Dolabella atque hinc† Antonius, inde 105  
 sacrilegus Verres referebant nauibus altis  
 occulta spolia et plures de pace triumphos.  
 nunc sociis iuga pauca boum, grex paruus equarum  
 et pater armenti capto eripietur agello,  
 ipse deinde Lares, si quod spectabile signum. 110  
 [si quis in aedicula deus unicus; haec etenim sunt  
 pro summis, nam sunt haec maxima. despicias tu]

Lines 111-112 appear bracketed in this way in Clausen. A favourite emendation is to read *iam* instead of *nam* in line 112. Courtney suggests, more radically:

AEDICULA and UNICUS are both excellent and add to the pathos of the picture ... in BICS 13 (1966) 40 I have proposed to read something like:

deus unicus: haec retinentes  
 pro summis (nam sunt haec maxima) despicias tu  
 forsitan

But, is emendation or deletion necessary?

2. Let us first notice the exact antithetical correspondence between 'the situation then' (100-104) and 'the situation now' (108-112); the rich house (*plena domus*) is contrasted with the 'poor acre' (*agello*); the natural produce of foreign countries, wool and shell-fish, transformed into luxury wares (*Spartana chlamys, conchylium Coa*) contrasts with the very Italian-sounding farm animals of the small-holder (*iuga pauca boum, grex paruus equarum, pater armenti*); the '*objets d'art*' of Parrhasius, Myron, Phidias, Polyclitus, Mentor (again foreign in resonance) contrast with the poor and very Roman *lar*. What item in the first leg of the contrast has no correspondent in the second? The pile of coin, *aceruos nummorum*.

3. Let us now read the passage again. Once the provincials had all these valuable things (100-105). Now they have these trivial trinkets (108-111). These are what pass for ... The natural expectation is ... riches, *diuitiae*.

4. Taking then 2 and 3 together, we see that the elaborated contrast and the run of the sense, if we pause at *summis* before running on, lead us to take *pro summis* as = *pro diuitiis* = *pro nummorum aceruis*, i.e. to take *summis* as the ablative plural of *summa*, 'a sum of money', a common enough word. That uncoupled *aceruos nummorum* of the 'situation then' finds its partner after all in the 'situation now', *aceruos nummorum* is picked up by *summis*. Compare the same coincidence of *summa*, luxury items, and *nummi* in lines 17-20:

ergo haut difficile est perituram arcessere summam  
 lancibus oppositis uel matris imagine fracta  
 et quadringentis nummis condire gulosum  
 fictile.

5. It is only as the reciter's voice - or the reader's eye - moves from *summis* to *nam sunt haec maxima* that the concluding *sententia*, completing the well-wrought rhetorical edifice, is seen to have at its heart a pun: *summis* looks back in the sense of 'sums of money'; it looks forward in the sense of 'the greatest', synonym of *maxima*.

#### E. A SUMMING UP

1. In six passages so far - five in this article, one in a preceding one (LCM 8.1[Jan.1983], 14-16) - I have tried to show that a Juvenalian climax consisted of a pun - or a play on the shifting meaning of words, if that formulation seems more respectable. In four of the passages considered in this article the Juvenalian *sententia* has been removed into editorial square brackets. In all six cases the interpretation I propose has always been 'available'. [What Professor Rudd, LCM 8.2(Feb.1983), 30, means by this word is uncertain. But readers who are intrigued will go to their Friedlaenders to find out what exactly of what I said in LCM 8.1 is to be found in Friedlaender.]

2. The question then is, why have these 'available' - and to my mind pretty obvious - interpretations not been adopted. In this respect, Professor Rudd's animadversion is most useful. For, after offering straightforward 'meanings' (x means this, and y means that) to words which I tried to show did not in their context have straightforward 'meanings', he offers a bridge over the gap between his meanings (not mine), and concludes: 'But it is hardly fair to Juvenal (my underlining) to claim it as a pun.' *hic jacet lepus* - 'hardly fair to Juvenal'. That preconception, based on the strength of received opinion, leads to impoverishment of Juvenalian exegesis, to the incarceration of the square bracket. Thence it is that not only can lines 2.149-159 be picked out to exemplify moral earnestness in Juvenal, but that they can be quoted in such a context shorn of line 159, shorn of *illic heu miseri traducimur* (see the Rudd and Courtney edition of 1, 3 and 10, p.7), i.e. shorn of the *sententia* crowning the build-up of lines 149-153, the *sententia* that gives those lines what meaning and flavour they have, a *sententia* too the meaning of which 'has been available for the past eighty years' - like Quartilla's secrets.

3. Professor Rudd finds my 'simple formula', 'the cleverness of the amoralist', an inadequate summing-up of 'these poems'. I am not such a scientist as to sum up 'those poems' on the basis of 11 lines. My 'simple formula' applied only to 11 lines. My remark was that it seemed to me to be the work of a perverse deity that the Rudd/Courtney commentary (p.7) had picked on that particular passage to exemplify something (the moral earnestness of Juvenal) which, to my mind, it was so far from exemplifying that it exemplified the opposite thereof.

4. Professor Rudd wishes the readers of LCM to consider the 'general point'. He refers to 2.153-158

and asks: 'Does he [Juvenal] really wish us to despise contemporary pathics in comparison with the heroes of old Rome?'. My view is that Juvenal does not raise the question. He is not concerned with it. That is the view I tried to document. The readers of *LCM* who take seriously the study of Juvenal (and those who come after them, if there be any such) will have to make up their minds on the evidence that is presented to them. But of one thing I am confident - they should not be asked to make up their minds on the passage 2.153-158; for that passage is a truncated excerpt. The fancifulness of 149-152 cannot be wished away; to cut off 159 from 153-158 is castration.

#### F. O LAMIA, LAMIA, WHEREFORE ART THOU LAMIA?

1. After 2.153-158, Professor Rudd refers to 4.150-152 and asks about it: 'does Juvenal really want us to condemn Domitian's reign of terror?'. Again I would say that Juvenal does not raise the question. Let us look at the three lines Professor Rudd refers to:

*atque utinam his potius nugis tota illa dedisset  
tempora saevitiae, claras quibus abstulit urbi  
inlustres animas impune et uindice nullo.*

2. There is no doubt that those lines sound very earnest and indeed are on a high poetic plane, *tota tempora saevitiae, claras inlustresque animas*, so poetic that we might think *impune et uindice nullo* not tautologous but rhetorically emphatic. But it is the grand style itself that should make us beware. *uindice nullo* does not simply repeat *impune*; it caps it with a pun on the proper name Vindex. Vindex rose against Nero. We are at the end of a 'cabinet' meeting which started at lines 37-38 of the satire with:

*cum iam semianimum laceraret Flavius orbem  
ultimus et caluo serviret Roma Neroni ...*

As the description of the 'cabinet' meeting starts with a comparison of Domitian to Nero, so it ends. Ferguson makes the connexion, and points to the pun in *uindice nullo*.

3. And the hypothesis is reinforced by the two lines that follow, two lines which again Professor Rudd would cut loose from the passage he would use to make his 'general point'. The last two lines of the satire run: *sed perit postquam cerdonibus esse timendus*

*coeperat: hoc nocuit Lamiarum caede madenti. 153-154*

As in passage 8.100-112 we looked at above (section D), we must study the contrast that is set here by Juvenal for us to explore. *perit* is picked up by *caede madenti*, *cerdonibus* by *Lamiarum*. So far, so good. Domitian frightened the *Cerdones*, they killed him; he killed the *Lamiae* - had they frightened him?

4. The contrast between *Cerdones* and *Lamiae* is the contrast between nobility and slaves or humble artisans (see the helpful and scholarly note by Courtney). But they have one thing in common, their ambiguity. *Cerdo* is a proper name or a common noun. Whether *cerdonibus* is 'a generalizing plural of a proper name, like *Virronibus* 5.149, and how far it had become a common noun is rather an unreal question', writes Courtney ad loc.. Indeed, Juvenal wants the status of the word to be ambiguous, so that we can see that *Lamiae* have an equally ambiguous status - the proper name of the Aelii Lamiae or a monstrous bugbear used to frighten children. We can now complete the contrast.

5. When dealing with the *Lamiae*, those frightening bugbears, Domitian was not daunted, and killed them. When he began to play the *Lamia*, to frighten *Cerdones*, they killed him. The antithetical frame, the ambiguous status of the word *cerdones* within that frame, the well-chosen phrase *esse timendus*, which points to the less obvious meaning of *Lamiarum*, end the satire with a word-play on proper names. The play on the name *Vindex* in the preceding line is but a foretaste, an eye-opener, for what follows.

6. To Professor Rudd's question, 'does Juvenal really want us to condemn Domitian's reign of terror?', I can only answer, 'Juvenal's main purpose in these lines seems to be to make puns on proper names'.

#### G. THE TRAITOR REVEALED

Who traduced Juvenal? If we accept the postulate that it is to be unfair to Juvenal to associate him with puns, we have perhaps defined a task for future Juvenalian scholarship. At some point in time, before the dawn of our manuscript tradition, in an unknown scriptorium, a monk, idle, possibly deranged, certainly well-versed in Latin, seeing the rivalry which the austere morality of Juvenal presented to the standards of his own unique religion, conceived the plan of discrediting him by peppering his text with superficial puns. To isolate that scriptorium, to track down that monk, to expose his name to obloquy - that must be the sacred task of scholars skilled in *Überlieferungsgeschichte*.

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I shall be referring to only three modern works: A.F. Garvie, *Aeschylus' Supplices: Play and Trilogy* (Cambridge 1969); O. Taplin, *The Stagecraft of Aeschylus* (Oxford 1977); and the edition of H. Friis Johansen and E.W. Whittle, *Aeschylus: The Suppliants* (Copenhagen 1980).

I. It was generally assumed even before the publication of the famous papyrus (P.Oxy. 2256 fr.3) that Aeschylus wrote a tetralogy consisting of *Supplices*, *Aegyptioi*, *Danaides* and *Amyclone*, and the hypothesis confirmed that the last two plays were indeed performed together. Some have claimed that the second play was not *Aigyptioi* but *Thalamopoioi*, but Garvie (187-191) effectively disposes of this (cf. Friis Johansen/Whittle I.23f.); it is doubtful whether such a play ever existed.

Taplin, however, (195-198) questions the whole tetralogy (though there is a sudden *volte-face* at the end of his discussion). This is a field in which scepticism is almost always salutary, but Taplin's is, I think, overdone, for he neglects the fact that *Supplices* itself obviously demands a sequel. Since the threat posed by the Aegyptiads is unresolved at the end, the play does not present a complete 'suppliant action' or a complete section of any kind; in fact it will stand by itself less well than any play of the *Oresteia*. And, if it belongs to a tetralogy, no one is likely to deny that it belongs to the same tetralogy as *Danaides* and *Amyclone*.

But Taplin is right to point out that *Aigyptioi* is a weak link. The natural assumption, if we leave aside remote possibilities, is that a play called *Aigyptioi* had one chorus, consisting of male Egyptians (Garvie 191f.). Subsidiary choruses are not to be multiplied unnecessarily, and we know of no play with a second chorus of comparable status and importance to the first. Also, if the Danaids formed a subsidiary chorus in *Aigyptioi*, why do the Aegyptiads not form one in *Supplices*? Nor is it much more attractive to suppose that the Danaids were transformed into mute extras for *Aigyptioi*, only to recover the power of speech in *Danaides*.

So what can the content of *Aigyptioi* have been? The Chorus cannot easily have fought a battle during the play, so the battle between Aegyptiads and Argives is generally placed between *Supplices* and *Aegyptioi* (Friis Johansen/Whittle I.50). Nor, obviously, can the Chorus have been murdered during the play, so the murder is placed between *Aigyptioi* and *Danaides*. Nor does it even seem possible that the murder was planned in *Aigyptioi*; 'The tragic chorus can be notoriously slow to understand', says Garvie (197), but the idea that the murder of Aegyptiads by Danaids was planned in the presence of the former and the absence of the latter will strain the credulity of most of us. Almost the only content which Friis Johansen and Whittle (I.50f.) can suggest for the play is negotiations between Danaus and the Aegyptiads, but, since the myth does not require that any such negotiations should take place, a play largely concerned with them will retard the action of the trilogy instead of advancing it.

In fact I find it difficult to believe that the murder occurred between *Aegyptioi* and *Danaides*, mainly because it is the central event of the myth - the event for which Danaids and Aegyptiads exist - and should no more fall between plays than the killing of Clytemnestra should fall between plays in an *Oresteia*. There is also the evidence of fr.124M. from the *Danaides*, which, though corrupt, appears to mention a morning song after a wedding night, and to contain a future indicative and an imperative. This could refer to the Danaids' second marriage (Friis Johansen/Whittle I.41), but the more natural inference is that it refers to their first (Garvie 230). It is suggested that the murder, though occurring between the plays, is not discovered until after the beginning of *Danaides*, and that the fragment belongs before the discovery, but it is hardly likely that the Danaids remained beside their murdered bridegrooms all night (asleep?), waiting for the murder to be discovered in the morning.

It is probable, then, that the fragment forms part of a list of arrangements for the wedding night, and that the murder occurred during *Danaides*. But this makes *Aigyptioi* even more of a yawning void. Whatever episodes we may be able to invent for it (Friis Johansen/Whittle I.50f.), it seems that, apart from providing a report of the battle, it can have done nothing to advance the essential action.

There is also a slight difficulty in the title. If the daughters of Danaus are *Danaides*, the sons of Aegyptus should be *Aegyptiadae*, not *Aigyptioi*. True, the sons of Aegyptus were also Egyptian (Garvie 189), but then so were the Danaids, and one would expect the distinct identity of this chorus to be marked by something more than the masculine gender. If *Aigyptioi* refers to the Aegyptiads, presumably the word is loosely used to mean Aegyptiads. This is by no means impossible, since titles were assigned haphazardly and sometimes ignorantly, as I hope to argue elsewhere; but it is another awkwardness in the orthodox account of the tetralogy. Taplin (197) toys with the idea that the title could have been *Aigyptos*, but this can surely be dismissed; the evidence for this title is weak, and the fact that *Supplices* barely mentions Aegyptus in his own right (only at 321-3, apart from such phrases as 'sons of Aegyptus') hardly suggests that he will be a prominent character in the next play.

All this falls far short of proving that *Aigyptioi* was not the second play; but, since we lack direct evidence that it was, it makes me want to look for alternative possibilities. Why should *Supplices* not have come second? A few scholars have believed this, and Garvie (185f.) has no difficulty in disposing of some of the reasons they give, but his only positive argument against their view is that 'It is impossible to see how the situation could be resolved in a single following play'. We have observed, however, that in any case almost all the essential action after *Supplices* must have occurred either during *Danaides* or between plays, so, if there is an impossibility, the

interposition of a vacuous *Aigyptioi* will do little to remove it.

But there is no impossibility. The action of *Danaides* could, for instance, have been as follows. Danaus and his daughters anxiously await the outcome of the battle. A messenger reports the Argive defeat and the death of Pelasgus. The Danaids lament and, together with Danaus, plan murder. The Egyptian herald arrives, lays down arrangements for the wedding night, and orders the Chorus off stage. Danaus prays to the gods. The Danaids re-enter, describe the murder they have committed, and denounce Hypermnestra. She enters and defends herself. Danaus or Hypermnestra having exited, the conflict is resolved by the intervention of Aphrodite, a prototype *dea ex machina*, who prophesies future events.

That is, of course, purely *exempli gratia*, but I see nothing wrong with it. Choruses do not commit murder off stage elsewhere, but they have no occasion to do so, and they do sometimes leave the orchestra for other purposes (Taplin 375f.). The handling of Hypermnestra is no more problematic on my view than on any other (cf. Garvie 207f.); I assume she was played by an actor and that the size of the Chorus remained unchanged, for twelve choreutae can as well portray forty-nine Danaids as fifty.

Other things being equal, then, it is much easier to suppose that *Supplikes* was followed at once by *Danaides* than that *Aigyptioi* intervened. But other things are not equal. If *Supplikes* came second, the first play was presumably set in Egypt, so its title was doubtless *Aegyptioi*. Those who have believed this seem always to have assumed that it concerned the quarrel between Aegyptus and Danaus, but against this it is sufficient to point out (Garvie 185f., cf. Friis Johansen/Whittle I 24) that the quarrel is unmentioned in *Supplikes*, for we expect some cross-reference between the plays of a trilogy. No one seems, however, to have asked in this connexion whether there is any other antecedent event on which *Supplikes* does lay emphasis. Why should I mention *Io*? Why indeed?

II. The parallels and contrasts between the myth of *Io* and that of the Danaids have often been pointed out, and are, indeed, implicit in the text of *Supplikes*. *Io* fled from Argos to Egypt, the Danaids from Egypt to Argos, and both found sanctuary on arrival. The problems of both were caused by male sexuality, but *Io* was finally reconciled to this, while all but one of the Danaids resisted it to the end. The gentleness of Zeus' touch contrasts with the violence threatened, and suffered, by the Aegyptiads. But despite this, the obsessive extent to which *Io* is dwelt on in *Supplikes* has always seemed odd. It will no longer seem so if Aeschylus is drawing attention to parallels and contrasts between two plays of his tetralogy.

It is no objection to point out that the Danaids lived five generations after *Io*, for the lapse of time is not nearly as great as that between *Prometheus Vincetus* and *Prometheus Unbound*. Nor can it be said that the myth of *Io* has little thematic connexion with that of the Danaids, for the text of *Supplikes* shows that Aeschylus did not think so. We can perhaps compare the relation between *Edonoi*, which concerned Lycurgus, and *Bassarai*, which seems to have concerned Orpheus (fr.83 M.). There is a further parallel between *Io*'s story and that of Amymone, who was also seduced by a god, and also became the ancestress of an illustrious line; and we may even guess from *Prometheus Vincetus* 652-4 that *Io*'s first union with Zeus (*Supplikes* 300f.), like Amymone's with Poseidon, took place at Lerna.

It is impossible to judge without subjectivity how much cross-reference is to be expected between plays. We may say, however, that *Io* is more prominent in *Supplikes* than Oedipus in *Septem* and less so than Agamemnon in *Choephoroi*, so her prominence falls within the expected range. Most of the references to her are in obscure and allusive terms. At *Supplikes* 291-313 we do find iambic trimeters going over ground that must, on my view, have been covered in *Aegyptioi*, but the passage is not so long that the repetition is offensive.

Strictly speaking the inhabitants of the Nilotic land should not be called Egyptians before the time of Aegyptus, but it is doubtful whether Aeschylus would have avoided the anachronism. Even if he did, the play could still have come to be called *Aigyptioi*, just as *Septem*, in which Thebes is not mentioned, came to be called *Hepta epi Thebas*.

In *Supplikes* *Io* is usually described as being completely transformed into a cow, while in *Prometheus Vincetus* she is a maiden with cow's horns, and this is often seen as a modification of the legend, designed to allow her to be presented on stage. By this argument, however, one might claim that Aeschylus too presented her on stage, since at *Supplikes* 568-570 he describes her as only partially transformed (Garvie 159f., Friis Johansen/Whittle on 569). I take it that her costume in *Aigyptioi* was in fact much the same as in *Prometheus Vincetus*, but that Aeschylus, in some passages, treated the cow's horns as a token of a complete metamorphosis, while the author of the *Prometheus Vincetus* described them more literally.

The real question, however, is whether *Io*'s arrival in Egypt provides enough material for a whole play. Here it must be remembered that, if we did not happen to possess *Supplikes*, we should be unlikely to believe in a play concerned solely with the Danaid's arrival at Argos. Judging from the analogy of *Supplikes*, the process of winning over the Egyptians and obtaining sanctuary could take up a good deal of the play; and *Io* will doubtless recount her wanderings at length. The Egyptians will at first react to her with horror, as we learn from *Supplikes* 565-70 (we now see why Aeschylus makes Egypt an inhabited country before *Io*'s arrival). Perhaps they will seek to cast her out (cf. Sophocles' *Oedipus Coloneus*). The King of Egypt (Nilus? - cf. Ps.Apollodorus II.1.4) may play a role, either persuading the Chorus to accept her or being persuaded by them. Zeus will have to make his purposes known, perhaps through an Egyptian priest



or priestess or a messenger from Ammon. Io will exit so that the miracle of Zeus' touch and her return to her normal form can occur off stage. These events will then be reported, perhaps by Ion herself or by a messenger, and the play will end with rejoicing and praise of Zeus. This is probably sufficient material for an Aeschylean play; and, if it does not sound like enthralling drama, it is always possible that Aeschylus was better at constructing plots than I am.

III. The author of *Prometheus Vincetus* certainly knew the Danaid tetralogy. This is shown by *Prometheus Vincetus* 853-869, where the Danaids are regarded as descendants of Io, given far more prominence than they deserve as a genealogical link between her and Heracles, and even described in an image borrowed from *Supplices* (857, from *Supplices* 223f.). The allusions to Io in *Supplices*, however, do little in themselves to explain why the author of *Prometheus Vincetus* should have chosen to bring her on stage, interrupting the story of Prometheus with something like a 'play within a play' (Taplin 265-7). If Aeschylus wrote a play about Io, however, the case is very different.

I imagine the author of *Prometheus Vincetus* reasoning as follows. 'I wish to write a play about the binding of Prometheus and a companion play about his release. These plays will have to be separated by many generations, but there is a precedent for this in Aeschylus' Danaid tetralogy. I shall be showing greater boldness than Aeschylus, however, as I always like to do, since the lapse of time between my two plays will be much greater. But Aeschylus had a ready-made link between the generations, since the Danaids were descendants of Io. Now one of the characters in my *Prometheus Unbound* will naturally be Heracles; and, if one thinks about it, the fact that the Danaids were descended from Io means that Heracles was descended from her also. Should I, then, bring Io into my *Prometheus Vincetus*? Yes, for if I do, there are other advantages. Aeschylus credited her with paltry wanderings, taking in the Thracian Bosphorus and Asia Minor. I can improve on this by making her cross the Cimmerian Bosphorus and wander to the ends of the earth, about which I happen to be so well informed. Moreover, Aeschylus drew an illogical moral from the myth, praising Zeus for releasing Io from torments that were Zeus' fault in the first place. By exposing the illogicality I shall also be contributing to my subversive characterization of Zeus.'

So there we have it -- speculation certainly, but not, I hope, entirely idle. It is sometimes more interesting to speculate than to respond to intractable questions as Housman's Chorus did: 'I have no notion why.'"

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ROBERT PARKER(Oriel College, Oxford): *A note on Aeneid 7.649-653*

LCM 8.10(Dec.1983), 160

650 *filius huic iuxta Lausus, quo pulchrior alter*  
*non fuit, excepto Laurentis corpore Turni.*  
*Lausus, equum domitor debellatorque ferarum*  
*ducit Agyllina nequiquam ex urbe secutos*  
*mille viros;*

These lines, with their repetition of the name Lausus, are an instance of the emphatic figure sometimes known as *epanaphora* (see e.g. Demetrius, *de eloc.* 61; cf. M. Lausberg, *Handbuch der literarischen Rhetorik*, Munich 1960, 314; Norden on *Aen.* 6.162-5). The figure had a famous prototype in *Il.* 2.671-4 (threefold repetition of Nireus) on which Demetrius comments: 'although Nireus is only mentioned once in the whole work we remember him as clearly as Achilles and Odysseus, who appear in almost every line' (loc. cit.; cf. Aristotle, *Rhet.* 1414a1-7). The name when repeated is normally accompanied by a description but does not govern a new verb (Housman on Juvenal 8.159-161, with eight other instances from Virgil). So, for instance *Aeneid* 6.162-5:

*atque illi Misenum in litore sicco,*  
*ut venere, vident indigna morte peremptum,*  
*Misenum Aeoliden, quo non praestantior alter*  
*aere ciere viros Martemque accendere cantu.*

Our attention can thus linger over the person and his attributes for a moment, since there follows a syntactical pause. The passage about Lausus, as printed above and in all modern editions, is an exception, but one that is easily removed by re-punctuating with a comma at the end of 650 and a strong point at the end of 651. The abrupt *ducit* in 652, without a connective or an anaphoric pronoun, is easy to parallel in this catalogue style, cf. e.g. 7.698, 748.

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CORRIGENDA TO KENNETH WELLESLEY(Edinburgh): *A privy council on the behaviour of freedmen*

(*Tacitus*, A.13.26.2) LCM 8.9(Nov.1983), 136-139

p.138 line 10 for 'an to iam' read 'iam to an'  
 p.139 line 5 for 'ultor' read 'ultro' and for 'sibi' read 'suam'  
 line 15 for 'in defendants' read 'if defendants'.